January 5, 1955

Dear Bruce:

I have had your draft for some few days now, but first got the time and courage to go into it summarily just now, I am quite optimistic that we can get together over it, though we may have (by ample precedent) some discussion over style. I think it should be possible to shorten the exposition considerably without impairing its clarity or content, but this will take some working out.

As to the data, taken overall, they parallel quite closely my own experience with SW-666, and except where some specific amendations or generalizations are needed, it probably will not be necessary to say more than that they accord. I do notice that you have had a few examples yourself of clones that were partly swarms, partly "unilines".

Would you consider some terminological rumination further? I am not very happy about "semi-clone", nor, much better, about unilinear, and am still trying to think of any better. How about some of the following: primogenitive, monochotomous, or (what I would vote for tentatively) (uni)-catenate. The last has the advantage of suggesting a number of correlated nouns and adjectives— chain (as the generalization of a trail); oligo-catenate; branched, etc. There are also a number of more or less precise analogues to catenate inheritance, for example Jennings, 1908, Jour. Exp. Zool. 5:577, which suggested this terminology. I am trying to collect instances for discussion.

Aside from style, the principal exception that I would take to the draft is that it is rather too peremptory in affirming the favored hypothesis. I am reminded of the fate of the original hypothesis of abortive transduction, which also seemed quite unequivocally supported. But as you will have a better opportunity to judge, I am sure we can agree about this, and without weakening the exposition. The historical development is not quite accurate (at least for my own psyche, and, e.g., I used trap droplets right from the start, and have had minimum trouble with inviability and sticking at early stages) but again some compromise is likely.

It will take me a little while to do all this, and there are also a few clenup experiments I want to try. It seems to me that the behavior of clones containing stable motiles should be studied farther (albeit with some difficulty). I am also suspicious that the distribution of particles from non-E parent cells is non-random, but it will take some rather tedious review of old protocols to find out for sure. What do you take as the critical evidence for the E-cell hypothesis, that you get only one cell per clone that gives more than 10 motile chains? But if the distribution of chains from non-E's is also non-random, we would not have a very precise test of the hypothesis.

I admit that this notion is far and away the most attractive, but we ought not go any further in advancing it than the evidence will allow-- do pardon that pomposity, Bruce, I just mean that precisely because the idea is intuitively attractive, we don't want to be caught holding a lemon. I wish I could think of feasible, critical experiments, but this is why I had been so hesitant about what ought to be a definitive publication.

Yours,